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It was decided to revise and enlarge the exchange list of the academy *Transactions*.

G. P. GRIMSLEY,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE ACTIVITY OF MONT PELÉE.

THE generally friendly tone of your reviewer's (T. A. J., Jr.) notice of my 'Mont Pelée and the Tragedy of Martinique' makes it almost ungenerous on my part to take exception to any of the statements that this notice contains. There is one point, however, dealing directly with the physics of Mont Pelée, that seems to me to deserve attention from its bearing upon volcanic phenomena generally. Your reviewer takes strong exception to the use that I have made of Russell's formula in computing the cubical content of the ash-cloud, and remarks that the defect in my reasoning 'lies in the assumption that a primary eruption is continuous for days or even hours.' The somewhat surprising statement follows that: "Professor Heilprin has failed to discriminate primary and secondary eruptions when he talks of Mt. Pelée 'being in a condition of forceful activity for upwards of 200 days.'" This does scant justice to my powers of observation, for it takes no scientist to separate or discriminate between the two classes of phenomena, any more than it requires a scientific eye to note the difference between the explosion of a dripping drop from a 'boiling kettle' and the 'blow' that issues from the snout. I fear that Dr. Jaggar has not seen Pelée in 'Pelée's glory,' otherwise he could hardly have hazarded the statement to which attention is called, and still less the subsequent one that 'the reviewer questions whether the volcano has been forcefully active from great depths for that many [200] minutes.' Had Mr. Jaggar been on the island of Martinique at any time during the days August 25 to September 3, inclusive, his conception of a 'primary eruption' would be very different from what it manifestly now is—he would have seen a raging central eruption continuous for that time, and not a

landscape of 'tremendous puffs that rise many thousand feet.'

When I prepared the chapter of my book which contains the calculations to which my reviewer takes exception, I was unaware of the conditions of the volcano which followed after my leaving the island. These are in many ways most interesting, and tend to confirm my conclusions as to the extraordinary quantity of the sedimental discharge from Pelée. The continuous activity of the volcano has been such that in the interval between the first week in September and the middle of December the mountain had increased its height by nearly or quite 900 feet (!), the needled summit of the cone (which had united with the old crater wall) being on December 16, as measured by Lacroix, 4,995 feet above sea-level. Much of this has since been destroyed, but Pelée is still at its work, adding to the 300 feet of ash that it has already laid down in parts of the valley of the Rivière Blanche. I do not think that the volcano can be seriously accused of working in working times of 'five or ten minutes.' In the days of the August-September activity, I feel satisfied—although necessarily lacking the means of *proving* the accuracy of my belief—that the continuous ash-discharge could not have been less than twenty per cent. of the measure of the steam-cloud; it may have been very much more.

ANGELO HEILPRIN.

PHILADELPHIA, PA.,
March 17, 1903.

THE PUBLICATION OF REJECTED NAMES.

I AM glad to see Mr. Bather's letter, although I can not altogether agree with what he says. My view is that if a description appears, accompanied by two or more names in the same publication, all being simultaneous in point of time, nothing but 'priority of place' can furnish a certain and invariable rule for selecting the one to be retained. I do not want to disturb existing rules, but I do want to see the same rules in use for all groups of animals and plants. My objection to the action of Messrs. Banks and Knowlton was based on the fact that they seemed to me

to err against the most generally accepted rule covering the particular matter discussed; and even if I grant, for the sake of argument, that this opinion was wrong, it still remains true that they unnecessarily created difficulties and left opportunities for an annoying divergence of opinion.*

Systematists might 'be much happier' for the time being if left to go their own ways, but the trouble would merely be thrown with increased force on the shoulders of those coming after. Dr. D. S. Jordan, when recently replying in *SCIENCE* to a criticism of mine, indicated the desirability of letting each case stand on the basis of the original publication, and not leaving the types of genera or species to be determined by the process of subsequent elimination. Now as a matter of plain common sense this is surely much to be commended, but if I adopt Dr. Jordan's view (as I should much prefer to do), what am I to do about the innumerable names of genera (especially among the *Lepidoptera*) which have been determined by the 'elimination process'? It is surely excusable to wish to be consistent.

Zoologists seem to be agreeing to the eminently sensible view that homonyms must be exactly alike, not merely similar. Botanists, however, have made and are making many changes on account of mere similarity in names. For example, *Batschia carolinensis* Gmelin, 1791, is a *Lithospermum*, and the name of the species is suppressed (being changed to *gmelini*) because of *Lithospermum carolinianum* Lamarck, which is an *Onosmodium*. According to my view the first mentioned plant should be *Lithospermum carolinensis* (Gmel.). Many names of genera, even in zoology, are changed for such reasons, and as the matter can not be yet said to be settled, I think it is worth while to make as strong a stand as possible for the rule 'no

homonymy without absolute identity of names.'

Zoologists generally agree that when subgenera or sections are raised to the rank of genera, the subgeneric or section names must be retained for the genera. Botanists, however, have frequently denied this altogether.

All these divergent practices are productive of future difficulties, and I can not see that anything is gained by going ahead with our eyes shut. Uniformity has to come, sooner or later.

T. D. A. COCKERELL.

A RARE SCIENTIFIC BOOK.

TO THE EDITOR OF *SCIENCE*: I would like information concerning the following very rare scientific book:

Purkenje: 'Commentatio de examine physiologico organi visus et systematis cutanei. Vratislav' (Breslau), 1823. Francis Galton states in 'Finger Prints' ('92), that there is *one copy in America*. As I am desirous of locating this or any other American copy, I shall be grateful to any one who can give me information on the subject.

HARRIS HAWTHORNE WILDER.

SMITH COLLEGE,
March 6, 1903.

SHORTER ARTICLES.

ORIGIN OF THE WORD 'BAROMETER.'

THE instrument familiar to us all as the barometer was first universally known by the name of its inventor as 'Torricelli's tube'; de Guericke, the inventor of the air-pump, called his huge water-barometer 'Semper Vivum,' also 'Weather Mannikin,' with the Latin form 'Anemoscopium.'

Soon after the year 1665 the words 'baroscope' and 'barometer' came into general use in England, but the individual to whom the credit belongs for originating these terms has not been certainly known; the assertion made by a contributor to the *Edinburgh Review* for 1812 that 'baroscope' was first used by Professor George Sinclair, of Scotland, in 1668, is an error, for both words occur in the *Philosophical Transactions* four years earlier. The passage is unsigned and reads thus:

* According to the plan indicated by Mr. Bather for saving the name *Cucumites lesquereuxii*, most published species would be nameless, as the name rarely occurs *after* the description! I should like to know what Mr. Bather thinks about the substitution of *Washingtonia* Raf., for *Osmorrhiza* Raf. as now adopted by American botanists.